

Pepper with a Pinch of Psalt

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I'm very happy to comment on Jack Marr's (1993) paper because I agree with many of his misgivings. I believe that *contextualism* and related ideas that discount mechanism and confuse subject and object are antidotes to clear thinking that undermine behavior analysis as a would-be science and seriously impede its advance. I'll comment on three topics raised by Marr's article: philosopher Stephen Pepper's contextualist epistemology, as explicated by Hayes, Hayes, and Reese (1988) and Morris (1988); context and history; and the mechanistic approach to behavioral development.

Contextualist Epistemology

What exactly is contextualism? According to Hayes et al. (1988) it is a metaphor: "The root metaphor of contextualism is the ongoing act in context." They go on to add that "Two fundamental categories of contextualism are *quality* and *texture*. Quality is the experienced nature of an act; texture is the details and relations that make up its quality. In contextualism, even these categories might change . . . because nothing is final or ultimate about our knowledge of the world—not even that the world will stay the same (p. 100)."

Contextualism is just one of four "world hypotheses," in Pepper's terms (the others are "formism," "mechanism," and "organicism"). "A world hypothesis is a model of the universe of observations and inferences. . . . Ideally a world view has unlimited scope and is so precise that it permits one and only

one interpretation of every event. In practice, all . . . current views fall short of this ideal" (Hayes et al., 1988, pp. 97–98). Finally, there is a problem in deciding between world hypotheses, because "each world hypothesis is autonomous . . . each world hypothesis creates its own field of play. Within that field of play 'competing' world views can be interpreted but cannot compete directly" (p. 98).

I must confess that all this gives me the greatest difficulty. Hayes et al. (1988) propose (a) that we accept as a guiding philosophy something that they themselves term a "metaphor"; (b) they go on to add that this metaphor cannot be tested in competition with other "metaphors": Evidence for one, they add, does not weaken any of the others. And (c) as for context, its meaning is not only vague, but is capricious: "even these categories might change . . . because nothing is final or ultimate about our knowledge of the world" (p. 100).

It seems to me that such a scheme is paralytically vague and, by its denial of competition between world hypotheses, effectively blocks any advance. Even Kuhn (whose work Hayes et al., 1988, cite with approval) acknowledges that competition between "paradigms" is the normal mechanism for scientific advance. Without competition—a systematic method of deciding between real alternatives—it is hard to see how any advance is even possible, much less likely. Consequently, the whole "Pepperian proposal" strikes me as so outrageous, so outside any of the worldviews proposed by and for natural science, that it is rather difficult to criticize. It is as if one were told that the world is run by invisible gremlins of great subtlety and erratic habits, who disappear or change their form whenever we train our measuring instruments on them. How would one refute such a view? Why would anyone hold it?

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Perhaps that is the best tack in rebuttal: Why should we believe Hayes et al.'s (1988) proposal? Is it true (in the sense of withstanding empirical and logical tests)? If not true, is it useful, and if so why? And finally, is it better than available alternatives? Let's take these questions one by one.

Is it true? First, their proposal is obviously a metaphysical, or at least an epistemological, one. You either buy Pepper's four world hypotheses or you don't. It is not possible to imagine any empirical test that would decide the matter. The same for the idea of "context as metaphor": Pepper was an aestheticist, not a scientist. For him, metaphors were the natural coinage of discourse, and imprecision was no defect. But a metaphor as vague as "context" barely qualifies as a scientific concept. Hence, we must either abandon it, or else move behavior analysis out of the domain of science—and into literary theory perhaps. This is not an outrageous suggestion: As Marr reminds us, *literata* Julie Andresen was invited to address the Association for Behavior Analysis in 1991! And critical theorist Barbara Herrnstein Smith's (1988) debt to Skinner is widely acknowledged. If that is the course Hayes et al. (1988) advocate, I have no more to say. Go ye forth and write critical theory—perhaps it will be less murky as penned by disciples of clear-writing B. F. Skinner!

The assumption that the world is subject to unpredictable change is also troublesome. Our *knowledge* changes, of course. A thousand years ago, few believed in atoms; 80 years ago, atoms were hard little billiard balls; today they are probabilistic clouds. But the evolution of science has always been sustained by a faith—yes, *faith*, it cannot be proved—that there is a single, unchanging, underlying reality, imperfect as our apprehension of it may be. I am inclined to go further and conjecture that this faith is an essential historical ingredient in the evolution of science itself. Notoriously, civilizations, even great civilizations, that lacked this belief—the cultures of the Indian subcontinent, for example—failed to develop science, despite high culture

of every other kind. Belief in a single, external reality may be essential to scientific advance. It has certainly proven useful so far. Yet Pepper's contextualism invites us to abandon it. Those of us who are conscious of history, and cautious by nature, cannot agree.

Is it useful? I am skeptical of a metaphorical worldview, but metaphors have an acknowledged value as scientific hypotheses, or as the source of such hypotheses. Some metaphors have proved to be useful and have led to useful experiments and hard theory (the idea of "field," for example); some were not so good (the idea of "ether," for example). It remains to be seen what great new theory will be the fruit of contextualism, and none of its proponents have exhibited experimental benefits that can be tied uniquely to it.

There are also problems with the contextualists' alternative to "truth," namely philosophical pragmatism. Believers in the pragmatic theory of truth, such as Hayes et al. (1988), Ed Morris (1988), and others, are unimpressed by any epistemological argument that appeals to the idea of absolute truth. "Successful working" is the catchword for pragmatists. How successful has the contextualist approach been? It is hard to measure "success" in science if views differ on what science *is*. Someone who eschews theory is unlikely to be impressed by theoretical advance, for example. Nevertheless, everyone will probably agree that the *ultimate* test for any science is control over nature. The great uncertainty is *when* the comparison should be made. As I pointed out recently (Staddon, 1993a, 1993b), successful working *now*—achieved by incremental improvements in known techniques—may block much more successful working down the road, attained, perhaps, through deeper theoretical understanding of the phenomenon. Until the race is really over, until we achieve more or less complete understanding of any subject, it is impossible to say which route—the pragmatic exploitation of known methods and frameworks versus exploration of alternative theoretical views—will be the ultimate winner.

"Successful working" is an unreliable criterion for assessing scientific progress, because we rarely know when the comparison should be made.

Perhaps measurement is easier in the applied arena, where the time frame is more restricted (did the patient get better or not?) and the criteria (what is "better"?) are less problematic. Certainly, contextualists seem to be more of a plurality in applied behavior analysis. My present comments are restricted to basic science, however.

It is certain that the behavior of children, the topic with which Morris (1988) is most concerned, is a great deal more complex than the behavior of pigeons. Attractively simple mechanistic accounts that seem to be within reach in some areas of pigeon behavior seem to fall far short of accounting for the evolving behavior of a growing child. This disparity between data and theory seems to have had one of two effects: to urge theorists to new and more sophisticated products, or to exacerbate dissatisfaction with the whole mechanistic enterprise. Followers of the latter course argue, like Morris, that it is not just this or that mechanistic theory that is wrong: it is the "mechanistic" theoretical enterprise itself. A few (non-behavior-analytic) developmentalists follow the former course, however, embracing a metaphorical form of nonlinear systems theory. I give a hypothetical example of this approach in the last section. But the contextualists seem to follow the latter course, throwing up their hands and heaving the baby—the theoretical enterprise itself—out with the bathwater—particular failed theories.

What are the alternatives? The alternative to contextualism is simply the standard "naive realism" of natural science: There is a real world out there—a real pigeon, rat, or child—and our business is to understand how it works. The contextualist may object that we, the experimenters, are also organisms, subject to the vaunted "laws" of behavior. We must (they contend) consider the implications of our laws for our own behavior. This is a surprisingly persuasive argument for many behavior analysts, but I

believe it also is mistaken. We are not only behaving organisms, we are also organic compounds and physical objects, but chemists and physicists seem to get along very well without worrying about it. One thing at a time: Let's understand rats and pigeons, and our human subjects, perhaps, before we worry about the grand self-reflexive implications of it all. The early biochemists would surely have been completely paralyzed if contextualist critics had insisted that they understand protoplasm and the composition of the blood before they had even got benzene right! Subject (the scientist) and object (the thing he or she is studying) are different, and we have no evidence that a strategy that blurs the difference will advance any science.

Context and History

"Context," as Marr (1993) says, has gotten out of hand. Originally introduced as a corrective to simplistic stimulus-response notions of behavioral causation, "act in context" has taken on a mystical quality that envelops all in obscurity. Sure, the same stimulus does not always produce the same response, and the same response topography does not always mean the same "act." But there are perfectly good mechanistic ways to deal with these facts without invoking the vague idea of "context." The key is provided by two complementary notions: *history* and *state*. It is the history of the organism prior to the act that can tell us how the act should be classified. (Was Rodney King really being beaten, or were they just love pats? Given that the preceding event was a car chase by police, we are inclined more to one hypothesis than the other.) It is the state of the organism that tells us whether a given stimulus will produce this or that response. Both these ideas can be found in Skinner, *state* early on (it was abandoned later), *history* throughout, but especially in his later writings (although its properties were never much spelled out).

I have explicated these ideas at greater length elsewhere (Staddon, 1993a, 1993b, in press), and will not spend much time

on them here. There is only one essential point: the complementarity of history and state. Once this is understood, the natural hostility of good behaviorists to mentalism and to "physiologizing" will lose its force.

The term *state* has many meanings, so it is essential to be clear on what a behaviorist might mean by the term. Some possibilities: In some absolute sense the organism's state is the instantaneous configuration of all its molecular constituents. That is not a helpful definition, because we haven't a prayer of measuring even a billionth of the things necessary to establish "state" in this sense. Another possibility is "mental state." What is the organism "thinking" now? Or, for the sophisticated cognitive scientist, what "mental representation" is "currently active"? That kind of state is not helpful either, for at least two reasons. First, we know that much is going on in our own brains that is not accessible to consciousness: creativity, the tip-of-the-tongue phenomenon, unexpected recall of something long forgotten—the list of manifestations of "unconscious" processes is endless. So mental state, defined in introspective terms, is certainly incomplete. Second, we know about the "mental state" of others only through observing their behavior. Why not, therefore (as Skinner so persuasively suggests) forget the idea of state entirely and just concentrate on the behavior? There is a third course, which I'll come to in a moment, but certainly Skinner's arguments should be sufficient (even if the numerous historical objections to introspection were not) to cause us to give up the idea of mental state, if not to accept his alternative.

So what's left? If not superphysiology (the physical condition of the brain) and not mental state, what other kind of state is there? The behavioristic answer is *state as equivalent history*. The idea can be grasped through the following imaginary dialogue:

A (a would-be contextualist): Why is the response to Stimulus A now not the same as it was yesterday?

B (theoretical behaviorist): Because the organism is in a different state now than it was yesterday.

A: How do you know?

B: Because (a) the response is different, and (b) the organism's history today is different from what it was yesterday.

A: But response (a) is circular, and of course the organism's history is different after an additional day, but so what? What is it about the additional day that has made the difference? Don't just tell me about a difference in "state" without saying something about the state that goes beyond the word.

B: You're right. What I need to add is that "state" is the name for the *class* of histories that are sufficient to produce the measured difference in response. As a good behaviorist, you should be comfortable with the idea of class membership as a way of defining behavioral concepts—remember Skinner's definitions of stimulus and response. Defining a state as a set of histories that are equivalent in terms of the future behavior of the organism is merely a modest extension of classical ideas.

A (persuaded): Ah!

The phrase *internal state* is sometimes used instead of *state*, which alarms many behaviorists. But *state* in the present sense is not necessarily either mental or physiological, as I have pointed out elsewhere (Staddon, 1993a, 1993b, in press; Staddon & Bueno, 1991). There is no cause for apprehension.

Pecking in Chicks:

A Developmental Example

What kinds of phenomena demand a contextualist account? Clearly Morris (1988) feels that developmental data demand something more than mechanism. In a section headed "Development as Response-Based Change Versus Structural Change," he writes:

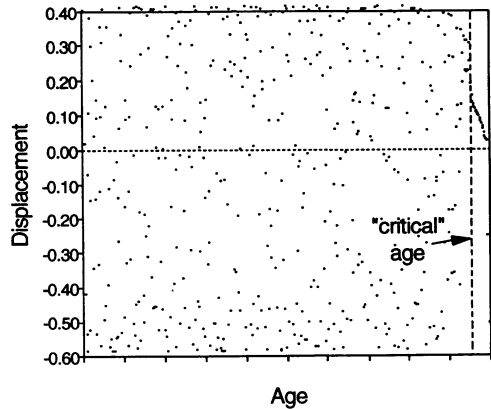
A second consequence of the mechanistic and contextualistic world views pertains directly to what it is that develops. From the mechanistic perspective,

development comprises changes in responding across chronological age. It represents a continuous, linear succession of cause and effect, wherein changes in responding are *reducible to, and exactly predictable from, their prior immutable forms*. In contrast, the contextual character of behavior analysis focuses on development as the ongoing evolution of interrelationships among stimulus and response functions in context—the interactions being mutual and reciprocal, and constitutive of behavior's structure. (p. 301, my emphasis)

I'm not quite sure what to make of this. But rather than becoming enmeshed in difficult prose, it might be more instructive to look at a very specific example of a developmental change. How might it be described from a mechanistic point of view? Obviously, my illustration will have to be simple: I'm trying to provide what mathematicians call an *existence proof*. If we can take a standard developmental phenomenon and show how it can be modeled adequately by a mechanistic system, then we have at least refuted the contextualists' claim that such phenomena are by their very nature incompatible with mechanism. Because contextualism involves a massive break with practices that are standard in natural science, it will be nice to show that, in fact, the break is not necessary.

My example is the old demonstration by Eckhard Hess (1956) that pecking in chicks improves in accuracy not because the chick improves its aim but because the scatter of the pecks simply decreases as a function of chronological age. The phenomenon is illustrated in Figure 1, which shows hypothetical data. The y axis is just the horizontal displacement of the chick's peck from a fixed grain target. Each point is a single observation, and the horizontal axis is chronological age. In the young animal, pecks are more or less displaced from the target in random fashion. But at a certain "critical" age, the variance declines to a low value.

Do these data conform to Morris's (1988) criterion? Well, successive displacements do not seem to be predictable from one another, so changes in responding are *not* "reducible to, and exactly predictable from, their prior immutable forms." The change from highly variable behavior to a rather invariant form is



quite sudden, which also seems incompatible with Morris's version of mechanistic theory as "a continuous, linear succession of cause and effect." Yet the data were in fact generated by a slight modification of the well-known logistic map (see Neuringer & Voss, 1993, for an accessible account). The change along the y axis corresponds to a continuous change in the "control parameter" in the logistic equation. The points were generated by a perfectly deterministic and mechanical process. Even simpler processes, like the ratio-invariance model of Horner and Staddon (1987), can also produce discontinuities like this. Ergo, there is nothing in principle unmechanistic about these data or, I would argue, any other developmental data. Hence, there is absolutely no reason for behavior analysts to abandon the tried-and-true philosophy of natural science in favor of a muddy contextualism. Math may be hard for most of us, but it is surely no more difficult than decoding sentences like the following:

The meaning of behavior emerges from the ever-evolving historic context (i.e., through historical causation) and is instantiated in the current context, as the present becomes past for subsequent behavior—hence the root metaphor of the "historic event" for the ongoing act-in-context. In the historic event, change is categorically given, thereby making the ontology of the psychological present both active and evolving, and obliging epistemology to be forever relative. (Morris, 1988, p. 299)

At the very least, fans of contextualism should be willing to present us with specific, quantifiable data as examples that

“cannot” be assimilated to a mechanistic account. Otherwise, we are simply trading a method that is at the root of modern civilization for an ill-defined philosophy with no scientific track record at all.

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